

hold that this relatively low value is the second point her paper has indicated for the first time. Meanwhile, I leave Mr. Hinks to consider whether the proven correlations of both proper motion and magnitude with parallax (under 0.4) have or have not any significant bearing upon the differential method of determining parallax, and upon the fact that more than 20 per cent. of negative parallaxes can be found.

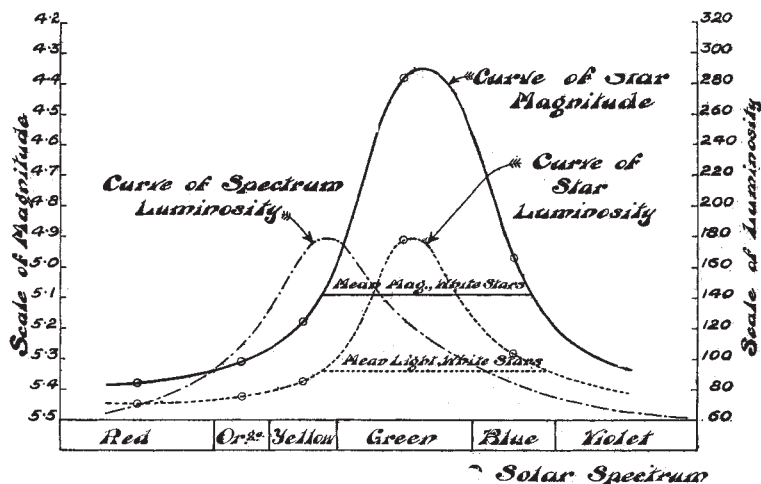
(c) The third point in Miss Gibson's paper was the statement that colours (and probably spectral classes) were more highly correlated with magnitude than distance. Mr. Hinks takes this point as one which will fully justify his criticisms at Leicester. I am of opinion that it is peculiarly a case in which he would have done well to have tempered his judgment by previously asking whether there was no method in our madness. He charges us with three grievous offences:—(1) using a highly selected material; (2) omitting to take into consideration the "white stars"; and (3) deducing from such material sweeping conclusions about the stars in general. He further charges me, *on the basis of this investigation*, with asserting "that colour and magnitude are related at least as closely as parallax or proper motion and magnitude."

In my letter, when making my statement, I made no reference to Miss Gibson's published work, but the fact that I cited the value of the correlation of magnitude and spectral class which is *not* given in the published paper might have warned Mr. Hinks that we held other reductions in our hands. Mr. Hinks asserts that our results would have been modified had we included the "white" stars. Using the 2834 stars of the Harvard Catalogue, of which roughly one-quarter are white ("Annals," vol. xiv.), Miss Gibson worked out more than a year ago the contingency of colour and magnitude; the value was 0.27 (± 0.01), as compared with the 0.30 (± 0.05) of the list in the Cape Observatory "Annals" previously given by her. Omitting the white stars from the Harvard data, the value is 0.297, agreeing absolutely with the result obtained from the Cape data. Thus we see that Mr. Hinks's suggestion that the Cape Catalogue is worthless, owing to selection of special stars, has no validity at all when we turn to the relationship of colour and magnitude, and, further, the inclusion of white stars produces, as we had logically anticipated, no sensible effect.

But I will go a step further, and reveal another conclusion, which I should naturally have preserved for the present, as the research is as yet incomplete. The mean magnitude of the white stars is almost identical with the mean magnitude of all the remaining truly "coloured" stars; the white star has not a preponderance of any special part of the colour spectrum, and if we wish to investigate the relationship between luminosity and colour we must logically leave out the white stars. The accompanying diagram gives the Harvard stars classed according to colour, with (a) the mean magnitudes of each colour group, and (b) the corresponding luminosity on the assumption that the light of a tenth-magnitude star is unity. It will be seen that the stellar luminosities form a curve very similar to the light-intensity curve of the solar spectrum, but shifted towards the violet end of that spectrum, possibly owing to the fact that the average star is hotter than our sun. On this scale there is clearly no place for the white stars, and the essential feature is that stellar magnitude takes its place in a continuous and definite relation to stellar colour.

I had no intention of anticipating work not yet completed, but Mr. Hinks's contemptuous reference to our omission of the white stars needed to be dealt with. Their inclusion or exclusion makes no difference from the standpoint of the statistical constant; their exclusion is, however, justified by the physical considerations which I have here suggested.

I should wish to say one word, albeit I am afraid it must be a strong one, about Mr. Hinks's further treatment of Miss Gibson and myself. In the paper, to use its own words, a suggestion, "even if it be only of the *vaguest* kind," is made that the bulk of the lucid stars may belong to a differentiated system. Mr. Hinks asserts that the basis for "this far-reaching suggestion" is one in which the white stars had no frequency in the record. Will the reader believe that this suggestion, which the writer herself describes as of the "vaguest kind," is not based on the colour correlation at all? Can Mr. Hinks really have criticised the memoir and supposed that even this vague suggestion was based on the 159 Cape stars? The suggestion, such as it is, is based on counts of *all* stars, and results from showing that a continuous curve can be found which describes with remarkable closeness the counts up to the sixth and seventh magnitude, but that beyond this magnitude any formula hitherto proposed fails even approximately to describe the frequency. This result, reached by other investigators, is confirmed by Miss Gibson, and in association with changes in other stellar characters, which occur about the same magnitude, does suggest, I venture to think, that in the *vaguest* kind of way some differentiation of the stellar system may possibly exist beyond the bulk of the lucid stars. I think Mr. Hinks owes us an explanation of what his statement, that a far-reaching suggestion has been based on



stellar statistics from which all white stars have been excluded, really is intended to convey.

In conclusion, may I say that I have learnt from my experience with biologists, craniologists, meteorologists, and medical men (who now occasionally visit the biometricians by night!) that the first introduction of modern statistical methods into an old science by the layman is met with characteristic scorn; but I have lived to see many of them tacitly adopting the very processes they began by contemning. Mr. Hinks is at present in the first stage; but may I remind him that even astronomy owes something to the layman, and express my hope that he may quickly reach a more understanding and sympathetic frame of mind?

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The Body of Queen Tii.

JUDGING from the letter addressed to NATURE of September 26 (p. 545), Mr. Hall (like Prof. Sayce in the *Times* of September 17) has been thrown into a state of doubt in regard to the real sex (? and age) of the mummy supposed to be "Queen Tii" by a letter from Mr. Theodore Davis, calling in question the accuracy of my statement that the mummy supposed to be an old lady of at least fifty years is the skeleton of a young man of about half that age.

Let me give a concise account of what I know in regard to this matter. At the end of June of this year Mr. Weigall, the Government inspector in the Service des Antiquités at Luxor, acting on the instructions of M. Maspero, the director-general of that department, sent me a skeleton to be examined and reported on. The skeleton was practically complete, for, although the face, certain ribs, and part of the pelvis were broken, most of the fragments were sent. Mr. Weigall told me that the bones were found in their coffin in a tomb opened by Mr. Theodore Davis in January last, and that they were supposed to be the remains of Queen Tii. Moreover, he has assured me that no possible mistake could have been made, because he himself and Mr. Ayrton had packed the bones, and they were received and unpacked by me in the anatomical department of the Cairo School of Medicine. The fact that the bones were soaked with paraffin wax, and that no other skeleton is known to have been so treated in Luxor, puts their identity beyond all doubt.

The skeleton is undoubtedly that of a young man of about twenty-five years of age.

It does sometimes happen that a skeleton presents features of such an indefinite character that even the most experienced anatomist hesitates before expressing an opinion as to sex; but these bones do not fall into such a category. All of them, and especially the skull, pelvis, and leg-bones, present the male characteristics in such a pronounced or even exaggerated form that a junior student of anatomy would be considered exceptionally stupid if he failed to recognise the sex. The skull is big and heavy-jawed, the frontal sinuses and superciliary ridges are exceptionally large, even for a man, and the mastoid processes are typically masculine; although the skull is exceptionally capacious, the face is disproportionately big. On the evidence of the cranium alone the sex is obvious.

The pelvis exhibits the most characteristic masculine features. The shape of the pubes and the pubo-ischial rami, the size and shape of the subpubic angle (67°), the form of the obturator foramen, the proportions of the pelvic cavity, and the shape of the iliac bones all conform to the definitely male type. The femur also serves to demonstrate the male sex in its size, inclination of shaft, and size of head.

Mr. Theodore Davis and those who have disseminated extracts from his letters have dealt rather unfairly with the two medical men, whose opinions they quote, in giving such wide publicity to statements which could have been made only in the most casual manner by anyone with any medical training whatsoever. It is so absurd as to be altogether incredible that "a prominent American obstetrician" would quote the figures 90° to 100° for the female subpubic (misquoted "pelvic" by Prof. Sayce and Mr. Hall) angle, and 70° to 75° as the average for this angle in the male, with the object of demonstrating the female sex of a pelvis the subpubic angle of which is only 67° !

But, quite apart from the very obvious male characters of the skeleton, there are even more obtrusive features equally fatal to the possibility of it being Tii's, which could hardly have escaped the observation of a medical man, however casual.

The teeth are practically unworn; three of the "wisdom" teeth had just been "cut," and the fourth was only just emerging from the jaw at the time of death; and a large number of epiphyses on ribs, vertebrae, clavicles, sternum, and pelvis were either separate or in process of joining. In other words, the bones are clearly those of a person of about half the age Queen Tii is known to have reached.

The archæological and historical remarks in Mr. Hall's letter do not concern me.

In a short time I shall publish a full account of this skeleton, with photographs exhibiting the evidences of sex and age, and the points of similarity and dissimilarity to the mummies of Amenhotep III., Yuua, Thua, Thothmes IV., and perhaps some other royal mummies of the eighteenth dynasty.

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Anatomical Department, The School of Medicine,

Cairo, October 4.

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The Interpretation of Mendelian Phenomena.

ALTHOUGH it is impossible within the limits of a short letter to attempt an answer to the question of the bearing of "Mendelism" upon biological problems in general, there are one or two points in Dr. Archdall Reid's letter in NATURE of October 3 which seem to require discussion.

Dr. Archdall Reid begins with the following statements:—"Mendelian phenomena are possible only when reproduction is bi-parental. They cannot occur, of course, when it is parthenogenetic." In the first of these statements the expression "bi-parental" should not be taken too literally, since in the majority of cases of Mendelian inheritance investigated hitherto the method of so-called self-fertilisation has been employed. I hope I may be pardoned for the assertion that the second statement is a little premature. For my own part I shall certainly await the result of experimental evidence upon the point before accepting it as conclusive.

In the absence of Dr. Archdall Reid's definition of what he means by "the problem of sex," I am not sure that I entirely understand the remainder of his first paragraph; but the suggestion may be made that "the function of sex," "the causation of variation," "retrogression of characters," and "mode of development" are less immediately to the purpose in the present condition of biology than the problems of the actual method of transmission of existing characters. Upon the problem of the "alleged transmission of acquisitions" Mendel's facts may even be said to throw some light; but in any case it seems rather severe treatment to belittle the importance of a biological discovery merely because it does not immediately lead to the solution of all the most difficult problems which biology affords.

Once more it must be repeated that the appearance of a blended first cross is no criterion of non-Mendelian inheritance. In the case of a problem like that of man, complicated as it is by the fact that he has "crossed more often than any other animal," and further rendered intractable by the circumstance that he is not amenable to experiment, a great difficulty arises in discovering which are the actual allelomorphs concerned. For these natural characters pay no heed to our definitions; so that if an investigator makes the mistake of first rigidly defining the "characters" with which he proposes to deal, and does not keep a perfectly open mind, prepared to revise his definitions in the light of the evidence which experiment alone can afford, he runs a great risk of finding only confusion where a proper analysis would have shown the presence of perfectly definite methods of inheritance. It would be extremely interesting to students of genetics to learn upon what evidence Dr. Archdall Reid bases his positive statement that there is no segregation in the case of the mulatto.

There is certainly occasion for surprise in finding it maintained that "nature selects only mutations"; but that natural conditions lead to the obliteration of a host of mutations is as fair a deduction from the fact that such mutations appear under cultivation as the current deduction that the conditions of cultivation actually cause the occurrence of this kind of variation. We have the testimony of de Vries and others that the former process actually takes place. That the latter process does so is an assumption which still lacks the support of facts.

R. H. LOCK.

Botany School, Cambridge, October 7.

The Colour of Dye Solutions.

It is generally accepted that the colour of dye solutions depends upon the chemical structure of the dye, and colour changes are usually attributed to some change in constitution; but certain recent investigations on colloidal solutions show that this argument must be accepted with caution. It is well known that colloidal solutions of the metals are highly coloured. Further, it is recognised that many dyes exist in solution in what, for lack of a better term, must be called the colloidal state. Some observations of my own point to the following statement as being true for certain dyes:—

The absorption spectrum of the dye in solution may be